The Impact of Enterprise Zones on Resident Employment:

An Evaluation of the Enterprise Zone Programs of California and Florida

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Abstract: This paper examines whether the enterprise zone programs of California and Florida affected the employment probabilities of zone residents. To do this, I develop a methodology for estimating the effects of programs in which selection for treatment occurs at the neighborhood level while the determination of the outcome of interest occurs at the individual level. This methodology is a combination of individual-level employment probability models and neighborhood-level propensity score matching. Studying programs that provided especially strong incentives to hire disadvantaged workers, I find no evidence that these enterprise zones impacted the employment of zone residents.

1. Introduction

In the last two decades the prevalence of enterprise zone programs has grown substantially as local, state, and federal policymakers look for ways to bring economic development to disadvantaged areas. One of the goals of these costly geographically targeted programs is to enhance job opportunities for people living in the zones. However, little is known about how enterprise zones affect resident employment. It is difficult to estimate the impact of these programs on the employment of people living in the zones for a number of reasons. In evaluating the effect of enterprise zones on resident employment, an individual-level process determines the outcome of interest (resident employment) while selection for treatment occurs at the neighborhood level. This differs from most program evaluation problems, where the process that determines the outcome of interest (e.g. wages) and the process that determines selection for treatment (e.g. receive training) happen at the same level of aggregation. Because of the unusual process for selection into treatment, standard program evaluation techniques have to be modified to estimate the effects of enterprise zones on resident employment.

I develop and implement a two-step methodology suited to the unusual selection process of these programs. The first step is to create a measure of the component of residents' employment probability that is explained by their neighborhood while controlling for the characteristics of area residents. To do this, I estimate the component of employment probability correlated with residential neighborhood, which I call the neighborhood component of employment. The next step is to estimate the effect of enterprise zones on resident employment by comparing the neighborhood components of employment of areas containing enterprise zones with those of comparable areas. This is accomplished with tract-level propensity score matching.

I apply this method to study the effects of the enterprise zone programs of California and Florida on resident employment. I find that a substantial portion of the variation across

neighborhoods in employment rates can be explained by controlling for the attributes of residents. This indicates that it is important to control for resident characteristics when making cross-neighborhood comparisons. Using propensity score matching, I find a pool of non-zone tracts that are observationally similar to tracts containing enterprise zones. I use these non-zone tracts to create an estimate of what the zones' neighborhood components of employment would have been in the absence of the programs. Even though I focus on two very targeted and generous enterprise zone programs, I find no evidence that the programs had significant effects on the employment of zone residents.

2. Background and literature review

Enterprise zones are programs where governments provide incentives for businesses to locate or expand in targeted, geographically defined areas. Often, states require that an area has below average economic growth and a mixture of residential and business land use in order to be designated an enterprise zone (Bondonio and Engberg, 2000). The incentives provided for businesses in the zones are most often a combination of property and income tax abatements, advantageous permitting and regulation, some infrastructure improvements, and tax credits for job creation (Peters and Fisher, 2002). By the year 2000, at least 39 states had enterprise zone programs and the Federal government had implemented similar programs, Empowerment Zones and Enterprise Communities (Bondonio and Engberg, 2000).[1] These programs had a number of goals, including increasing cities commercial tax bases and attracting new jobs to the zones.

One of the motivations for these geographically targeted economic development programs is to improve the employment outcomes of people living in the targeted areas. People, particularly those with low skills, do not readily move in order to find work (Bartik, 1991). The spatial mismatch literature has shown that many workers live far from the businesses most likely to employ them (e.g.

Kain, 1968; Holzer, 1991; and Ihlanfeldt and Sjoquist, 1998). Enterprise zones seek to alleviate these problems by encouraging job growth in neighborhoods with unemployed workers.

I focus exclusively on the enterprise zone programs of California and Florida from the period 1986 to 1990. These programs share a number of features that make them interesting and feasible to study. Both states designated enterprise zones in areas with high poverty and unemployment rates.[2] The number of enterprise zones in each state is sufficient to allow me to provide separate estimates by state. Also, detailed maps that show enterprise zone locations are available for both states.

The primary reason to focus on these programs is that both offered businesses in enterprise zones generous hiring tax credits. In California, the tax credit allowed businesses to reduce their tax bill by as much as 50 percent of the wages paid to workers enrolled in specific job training or welfare-to-work programs. In Florida, businesses hiring eligible workers could choose between a corporate income tax credit of 15 percent of wages up to 1,500 dollars per month or a monthly sales tax credit of 100 dollars for each full-time hire and 50 dollars for each part-time hire. Hires who were zone residents, welfare recipients, or who were previously participants in Job Training Partnership Programs were eligible for Florida's hiring tax credits.[3] In the first three years of the California enterprise zone programs 6.6 million dollars were spent on hiring tax credits, representing two-thirds of the enterprise zone tax credits documented by the State of California (California Legislature, 1999). The State of Florida had 21.3 million dollars of enterprise zone hiring tax credits from 1987 to 1990, which represents four-fifths of the enterprise zone tax credits provided by the state during the period (Office of the Auditor General, 1993).

Economic theory suggests that enterprise zones can increase zone resident employment. In zones where subsidies are provided for hiring zone residents, businesses have an incentive to hire zone residents rather than non-residents because the net cost of compensation for zone residents

would be lower than for non-residents. In addition, the subsidies might be used to increase the wages of existing jobs for zone residents, perhaps causing wages to rise above individuals' reservation wages and moving them into employment. Even if enterprise zones do not have specific incentives for hiring zone residents, one would still expect a rise in zone residents' employment probabilities if the policies lead to additional jobs locating in the area.

There is also theoretical basis to think that resident employment probability might not rise even if the enterprise zone programs increase the number of jobs in the area. If the subsidies offered in enterprise zones attract businesses that require workers with skills not possessed by zone residents, then there would be no boost to zone resident employment probability. This might be the case because the value of enterprise zone incentives differs greatly from establishment to establishment (Papke 1994). A labor-intensive establishment may find little value in capital or property based incentives, while a capital-intensive establishment may find little value in payroll-based incentives. Another way that the enterprise zones could reduce the employment of zone residents is by inducing investment into capital that is a substitute for the labor of zone residents, which would reduce demand for the labor of zone residents.

Enterprise zones could also have no effect on resident employment if the incentives were poorly targeted or insufficiently large to induce businesses to increase zone resident employment. Therefore, depending on how businesses respond to the zone incentives, enterprise zones could increase, decrease, or not affect the employment of zone residents. I estimate the net effect of these various influences of enterprise zones on zone resident employment probability.

There is a large body of research evaluating the effects of enterprise zones. The early literature, which found widely varying effects, is well reviewed by Wilder and Rubin (1996) and Peters and Fisher (2002). Boarnet (2001) details the limitations of many of these studies: poor data,

inability to compare enterprise zones to similar areas that were not designated as zones, and little guidance provided to policymakers about how to increase the effectiveness of the programs.

Recent work by Bondonio and Engberg (1999), Bondonio and Greenbaum (2007), Engberg and Greenbaum (1999), Greenbaum and Engberg (2000), O'Keefe (2004), and Rogers and Tao (2004) address two of Boarnet's (2001) concerns by using more geographically detailed data and systematically comparing outcomes in enterprise zones to those in comparable areas. Four of these recent studies focused on the impact of enterprise zones on the number of jobs located in the zones. Bondonio and Engberg (1999) and Greenbaum and Engberg (2000) found that zones do not have significantly greater employment growth than comparable areas that do not have enterprise zones. Bondonio and Greenbaum (2007) find some evidence that expanding businesses located in enterprise zones grow faster than comparable businesses outside of enterprise zones. O'Keefe (2004) finds that during the 1990's enterprise zones in California had 3 percent faster employment growth in the first six years after designation than did comparable areas.

While there have been many studies of whether enterprise zone programs attract jobs to the zones, there have been few studies of the effect of enterprise zones on resident employment. Using area fixed effects to control for the characteristics of zones, Papke (1993) finds that enterprise zones in Indiana increased zone resident employment by approximately 1.5 percentage points. Studying six states, Greenbaum and Engberg (2000) find mixed results that suggest that enterprise zones have no significant effect on the growth rate of resident employment and, counterintuitively, that zones increase the growth rate of unemployment. Rogers and Tao (2004) find that small cities in Florida that contained enterprise zones had a greater reduction in their unemployment to population ratio from 1980 to 1990 than did other small cities that qualified for zone designation, though the difference is not statistically significant.

Due to data limitations, most studies of the impact of enterprise zones on employment use cities or Zip Codes as the unit of analysis. This is unfortunate because these can be a poor measure of where enterprise zones are located. In California and Florida, defining zone areas as all Zip Codes containing an enterprise zone yields areas that are, on average, six times the actual zone size. It is interesting to note that one study which finds that enterprise zones had substantially faster employment growth than comparable areas, O'Keefe (2004), uses a more geographically detailed unit of analysis, 1990 Census Tract. I use 1980 Census tract as my unit of analysis to have the maximum possible geographic detail and time consistency.

The main contribution of this study is that I condition both for zone resident characteristics and the zone designation process. Comparing unconditional employment rates across neighborhoods may be misleading for several reasons. Relative to characteristics such as race, marital status, or education, neighborhood is a weak predictor of employment. Therefore, small differences in the demographics of neighborhoods could lead to large differences in neighborhood employment rates. Another issue is that the data best-suited to measure neighborhood-level employment rates, the full long form samples of the Decennial Censuses, can suffer from small sample distortions for very detailed definitions of neighborhood. Conditioning on the characteristics of residents can reduce these distortions. Therefore, generating neighborhood-level employment estimates that are conditional on resident characteristics facilitates cross-neighborhood comparisons. Because enterprise zones in California and Florida have much higher poverty rates, unemployment rates, etc. than most other areas, it is also important to compare enterprise zones to observationally similar areas in order to generate estimates of what would have happened without the zone programs. The econometric technique discussed in the next section addresses these concerns.

3. Methodology

This section presents a three stage estimation strategy that blends employment probability models with neighborhood-level propensity score matching to get estimated effects that are conditional on both resident characteristics and the selection of neighborhoods into enterprise zone designation.[4] The first stage uses employment probability models in order to calculate the component of employment probability that is correlated with neighborhood conditional on the characteristics of the people who live in the neighborhood. The second stage estimates the propensity for an area to be designated an enterprise zone. The third stage estimates the effect of enterprise zone policies on resident employment by matching on the estimated propensity scores.[5]

The parameter of interest in this study is the average effect of being designated an enterprise zone on resident employment probability for areas designated as zones, conditional on the traits of residents. This is also called the average treatment effect on the treated, where the treatment for a neighborhood is being designated an enterprise zone. More formally, the parameter of interest is:

$$\Delta = E[Y_{1j} - Y_{0j} \mid T_j = 1, X_j = x_j], \tag{1}$$

where T_j=1 if area *j* contains an enterprise zone, Y_{0j} is the employment rate in area *j* in the absence of an enterprise zone, Y_{1j} is the same with an enterprise zone, and X_j is a vector of the demographic characteristics of the people who live in area *j*. What makes this non-trivial is that it is not possible to simultaneously observe Y_{1j} and Y_{0j} for the same area and that it is necessary to condition on X_j. If it were possible to observe both Y_{1j} and Y_{0j} I would estimate:

$$y_{ij} = F\left(\beta X_{ij} + \delta T_j + \gamma_j + \varepsilon_{ij}\right), \tag{2}$$

where *i* indexes individuals and *j* indexes Census tracts, $y_{ij} = 1$ if individual *i* in *j* is employed and 0 otherwise, X_{ij} are a set of observable characteristics of individual *i* in *j*, γ_j is a tract fixed effect, $F(.)$ is a monotonic function (such as the Normal cumulative density function), and ε_{ij} is an error

term. The estimate of the average treatment effects of enterprise zones on resident employment would be:

$$\Delta = E\left\{[F(X_{ij}\beta + \delta + \gamma_j) - F(X_{ij}\beta + \gamma_j)] \mid T_j = 1\right\}. \tag{3}$$

Because it is not possible to observe the enterprise zone tract outcomes without enterprise zones, a different method is necessary.

I generate treatment effect estimates using propensity score matching.[6] The vast majority of neighborhoods are not similar to enterprise zones, so most non-treated areas provide little information about what would have happened to enterprise zone areas in the absence of the programs. Propensity score matching systematically selects relevant comparison areas. Enterprise zones would be outliers in most regressions of area traits on employment outcomes because they are so disadvantaged. Therefore, models that fit most areas are likely to fit poorly for enterprise zones. Matching estimates do not suffer from this problem because matching does not impose a specific functional form on the relationship between observable characteristics and the outcome of interest.

To use propensity score matching estimators it is necessary to assume that: selection into treatment is a function of observable characteristics Z_j, there exists a set of observations *S* with similar Z_j that contain tracts with $T_j = 1$ and tracts with $T_j = 0$ (the common support condition), $\gamma_j \perp T_j \mid Z_j \in S$ (the conditional independence assumption), and there exists a function $P(Z_j)$ such that $P(Z_j) = \Pr(T_j = 1 \mid Z_j)$. These conditions are more completely presented in Heckman, Ichimura, Smith, and Todd (1998). Then by propositions one and two in Rosenbaum and Rubin (1983), it follows that $\gamma_j \perp T_j \mid P(Z_j), Z_j \in S$. Therefore,

$$E[\gamma_j \mid T_j = 1, P(Z_j), Z_j \in S] = E[\gamma_k \mid T_k = 0, P(Z_k), Z_k \in S]. \tag{4}$$

This yields an estimate of a zone tract's employment fixed effect in the absence of the zone program.

To implement this methodology, I substitute $\alpha_j = \delta T_j + \gamma_j$ into (2) and use a probit to estimate the individual-level employment probability regression:

$$y_{ij} = f\left(\beta X_{ij} + \alpha_j + \varepsilon_{ij}\right).^{7} \tag{5}$$

Note that $\alpha_j = \gamma_j$ if $T_j = 0$ and that $\delta = \alpha_j - \gamma_j$ if $T_j = 1$. I estimate the tract-level propensity score function $P(Z_j)$ with a probit and get the estimated propensity score function $\hat{P}(Z_j)$. The estimate of the tract fixed effect for a zone tract in the absence of the program is:

$$\hat{\alpha}_j^M = E[\gamma_j \mid T_j = 1, P(Z_j), Z_j \in S] = \sum_{k \neq j} w(\hat{P}(Z_j), \hat{P}(Z_k))\hat{\alpha}_k, \forall k \in S \text{ and } T_k = 0, \tag{6}$$

where $w(.,.)$ is the Epanechnikov kernel weighting function.[8] The Epanechnikov kernel estimator creates a composite control based on a number of non-treated areas, where the contribution of each non-treated area is determined by its similarity in propensity score to the treated area. The estimate of average treatment effect on the employment probability of residents in tracts that were designated enterprise zones is:

$$\hat{\Delta} = \frac{1}{H} \sum_{j \in T_1} \left\{ \frac{1}{N} \sum_{i=1}^{N} \left[F(\hat{\beta} X_i + \hat{\alpha}_j) - F(\hat{\beta} X_i + \hat{\alpha}_j^M) \right] \right\} \tag{7}$$

where T_1 is the set of tracts designated as enterprise zones and able to be matched, H is the number of treated areas, N is the number of individuals in the sample, and $\hat{\alpha}_j, \hat{\beta}_j,$ and $\hat{\alpha}_j^M$ are estimates from (5) and (6).

The estimate of the average treatment effect on the treated, $\hat{\Delta}$, ignores general equilibrium effects, so the stable unit treatment value assumption must hold for all areas in the analysis (Lechner 2001). Therefore, it must be assumed that individual characteristics do not change as a result of living in a treated area. This assumption requires that the zone programs do not induce selective

migration. In work not reported here, I find that migration patterns in enterprise zones did not differ from those in similar non-zone areas, which supports this assumption.

Of the assumptions necessary to use propensity score matching, the assumption that selection is strictly on observable characteristics usually raises the most concern. It is possible that unobservable characteristics influenced which of the areas that met the states' criteria were designated enterprise zones. However, as Bondonio and Engberg (1999) note, enterprise zones were designated by state governments in accordance with policies that outline specific levels of poverty, unemployment, or other observable characteristics required for eligibility. In California and Florida, much of the legislated selection process depended on data from the 1980 Census of Population and Housing similar to that which I use to estimate the propensity scores. Therefore, the concern about selection on unobservable characteristics is less problematic than in many other contexts.

In both California and Florida the observable characteristics that I control for effectively control for the observable component of the enterprise zone selection process. However, there are also areas that were not designated enterprise zones with characteristics similar to the zone tracts. Both states had a cap on how many zones could be designated and some applications for designation were rejected. Also, some areas eligible for zones did not apply (Rogers and Tao, 2004). It is likely that the main causes of applications being rejected and eligible areas opting not to apply are unobservable political processes. Local governments that are especially active at pursuing new sources of revenue may be more likely to apply. In addition to the observable characteristics that I control for, the incentives that local governments proposed to offer and the quality of their proposals were factors in the selection of zones. There is no clear link between having an activist local government and resident employment probability, especially after controlling for the socio-economic characteristics of an area. Therefore, it is reasonable to assume that the variation in zone

designation after controlling for observable characteristics is independent from resident employment probabilities.

The standard errors of the matching estimates are the result of bootstrapping the propensity score and matching stages with 300 replications.[9] Because the individual-level employment probability models with area fixed effects are computationally intensive, it is impractical to bootstrap over the first stage.[10] In work not reported here that uses tract-level employment probability models for the first stage, I found that bootstrapping over all stages yields standard errors very similar to those when bootstrapping only the second and third stages. This suggests that the standard errors presented are a good approximation of the true standard errors.

4. Data

The methodology I use is data intensive. It requires individual-level data from a period after the designation of the zones, neighborhood-level demographic and economic data from prior to the designation of zones, and data on the location of zones. Since earlier estimates of the effect of enterprise zones on employment suggest that any effects are small, it is very important to minimize measurement error by using the same detailed geographic definition in both the pre- and post-designation periods. For this reason, I use 1980 Census tract – Census Place combinations (which I call tracts) as my definition of neighborhood.[11]

The post-designation data that I use are the restricted access individual-level microdata from the 1990 Decennial Census of Population and Housing. Because this data has geographic units that are finer than 1980 Census tract, I can use 1980 Census geographic definitions for the post-designation period as well as the pre-designation period. The pre-designation neighborhood-level data comes from the Summary File Three from the 1980 Census of Population and Housing (U.S. Department of Commerce, 2001c). To measure economic growth immediately before the period when zones were designated, I make city-level tabulations of the number of jobs and establishments

for each year from 1982 to 1986. The source data for these tabulations are the annual Standard Statistical Establishment Lists, restricted access establishment-level databases maintained by the Census Bureau.[12] These are the data that are used as the sample frame for Census establishment surveys and are the source data for the County Business Patterns series.

Without access to the restricted access microdata, I would have to use less precise geographic definitions and would induce substantial measurement error by converting data from one unit of geography to another. There is some error in measuring zone residence status even when using Census tracts; on average, less than half of the 1990 population of tracts that contain enterprise zones actually lives in the zones. To reduce error in measuring who is a zone resident, tracts that contain a zone but where less than 25 percent of the population lives in a zone are dropped from my sample. For the remaining tracts that contain a zone, over 70 percent of the 1990 population lives in a zone.

Some sample restrictions are made to focus on areas similar to enterprise zones and to reduce measurement error in the tract-level variables. Since nearly all of the zone tracts in California and Florida are in urban areas, tracts where less than 95 percent of the 1990 population lived in an urban area are dropped, which eliminated 15 percent of the tracts in the sample. In order to eliminate isolated urban areas that would not be appropriate controls for enterprise zones, the analysis was restricted to tracts located in a Metropolitan Statistical Area (MSA), which eliminated 295 tracts. Tracts with 1980 population below 100 are dropped to reduce problems with measurement error and missing data. This cut 662 tracts from the sample. In addition to these restrictions, the sample for the individual-level employment probability model is restricted to adults aged 18 to 55 and not enrolled in school in order focus on prime-age employment.[13]

The matching stage requires some additional restrictions. Areas with suppressed data for the items included in the propensity score models are dropped from the sample. Fixed-effects estimated

with very small samples have high variance and the point estimate may be far from the true effect.[14] For this reason, the matching sample is restricted to those tracts with at least 30 residents in the sample for the employment probability models. One criticism of enterprise zone policies is that they may draw business development away from areas near but not in the zones because of the potential for business to receive zone benefits for a small change in location. If these negative spillovers exist, using tracts near enterprise zones as matches for zone tracts may overstate the effect of zone programs. To avoid this problem, I exclude all non-zone areas that are fewer than five miles from any enterprise zone.[15] In California, the sample used for propensity score matching has 4,229 non-zone tracts and 91 zone tracts. In Florida, the corresponding numbers are 1,164 and 97.

5. Preliminary stages

The first stage of the methodology estimates the tracts' components of employment probability with individual-level employment probability models with tract fixed effects. The results from these models, found in Table 2, follow familiar patterns: employment probability rises with education, women's employment probability falls as the number of related children rises, employment probability is lower for blacks, etc. The estimates from the models with only men in the labor force are similar to those for men from the models that pool men and women. Using the samples that include men who are not labor force participants, I estimate the joint probability that a man is in the labor force and employed. These results, in the "All Men" columns of Table 2, also look typical: the signs are the same as in the employment probability model for men in the labor force, but the magnitudes are greater. The only anomalous result is that, in all of the relevant models, men in California with less than a grade nine education are more likely to be employed than equivalent men who completed at least grade nine but dropped out of high school. In work not reported here, I have estimated employment probability models without fixed effects for all of the samples. These models indicate that the marginal effects that are most sensitive to the inclusion of

the tract effects are the race indicator variables, which are about 40 percent smaller in the models with fixed effects. This is not surprising given the degree of residential segregation in urban areas.

The fact that enterprise zones are more disadvantaged than most other areas is clear from the summary statistics in Table 3.[16] For all of the measures of socio-economic conditions, enterprise zone tracts fare worse than non-zone tracts. For example, in both California and Florida enterprise zone tracts have poverty rates more than three times those of non-zone tracts. The share of adults with any post high-school education in zone tracts is less than half as high as in non-zone tracts. Zones are located in cities that had slower than average employment growth from 1983 to 1986.

Propensity score matching controls for these differences by matching zone tracts with non-zone tracts that have similar propensities to contain a zone and therefore similar observable characteristics prior to the designation of the zones. The models of the propensity for a tract to contain an enterprise zone are presented in Table 4. These models show that California's selection process focused on choosing areas with high rates of poverty and public assistance but was weakly influenced by job growth rates, housing unit vacancy rates, or the age of housing units. In Florida, nearly all measures of neighborhood socio-economic conditions had the expected effect on the propensity of containing a zone. These models yield the propensity scores I use for matching – the tracts' predicted probabilities of being designated as part of a zone.

To be appropriate for matching, propensity scores need three properties.[17] First, they need to fit the process that determines whether an observation is treated (in this case, contains a zone). Second, there has to be sufficient overlap between the propensity score distribution of treated and non-treated observations so that the resulting treatment effect estimates are informative. Finally, the propensity scores have to serve as balancing scores, meaning that on average matched treated and non-treated observations are observationally equivalent for the characteristics that determine both the outcome of interest and selection into treatment.

The propensity scores I use have these properties. Figures 1(a) and 1(b) show the distributions of propensity scores for, respectively, California and Florida. Over 88 percent of non-zone tracts have propensity scores at or below 0.05, which indicates that the models do a good job fitting the tracts that do not contain an enterprise zone. Even though the majority of non-zones tracts have very small probabilities of containing a zone, most zone tracts have non-zone tracts that are sufficiently close to permit matching. This can be seen the fact that there are non-zone tracts throughout the propensity score distributions. Comparing the "Matched" columns of Table 3 shows that, on average, zone tracts and non-zone tracts that are matched have very similar characteristics. The difference between the means of the matched zones and non-zones is always less than 0.3 times and usually less than 0.15 times the corresponding standard deviation for all characteristics in the propensity score model. This shows that the propensity score matching successfully balances observable characteristics between the zone and non-zone tracts.

6. Estimated effects of enterprise zones on resident employment

Before moving on to the treatment effect estimates, it is interesting to consider the unconditional differences in unemployment between zones and other areas. The means of 1990 employment outcomes are at the bottom of Table 3. In California, the average employment rate of people in the labor force is 9.8 percentage points lower in zone tracts than in non-zone tracts. In Florida, the employment rate in zone tracts is 9.4 percentage points lower than non-zone tracts. The difference in men's employment is approximately a percentage point less negative in both states. The other employment outcome I consider is the neighborhood's component of employment, which is the tract fixed effect from the individual-level models mapped into marginal effects.[18] The mean difference between zones and non-zones in the neighborhood component of employment is -4.2 percentage points in California and -3.4 percentage points in Florida. This shows that more than half

of the difference between zones and non-zones in employment probabilities is explained by resident characteristics.

The matching estimates of the average treatment effect on the treated of enterprise zone designation on resident employment are in Table 5. All of the estimated effects are negative. The first set of estimates (row a) are for men and women pooled together and use propensity scores estimated with data from the 1980 Decennial Census and the Standard Statistical Establishment List. In California, the estimate of the effect on the raw employment rate is -0.6 percentage points and that for the effect on the neighborhood component of employment is -0.38 percentage points. In Florida, the estimate of the effect of enterprise zones on raw employment is -1.52 percentage points and is -1.03 percentage points when conditioning on resident traits. In both states, the estimated effects of enterprise zones on resident employment are less negative when controlling for the characteristics of the people living in the zones. In Florida, conditioning on resident characteristics attenuates the estimate of the reduction in resident employment by over 30 percent.

None of these employment probability effects are significantly different from zero, but the estimates of the effect on the neighborhood component of employment are more precise. For example, the 95 percent confidence interval of the estimate of the effect of Florida's zone on raw employment ranges from -4.60 to 1.55 while the confidence interval for the effect on the neighborhood component of employment ranges from -3.01 to 0.95. This increased precision is one of the benefits of controlling for the confounding effects of resident characteristics.

The results above match zone and non-zone tracts based on propensity scores estimated with only characteristics from the 1980's. If there are long term trends in the prosperity of neighborhoods, the results above may be misleading because I do not control for such trends. To address this concern, I add tract characteristics from 1970 to the propensity score model and repeat the propensity score estimation and matching stages. The resulting propensity score models are very

similar to the ones discussed above. Fewer enterprise zone tracts can be matched when 1970 characteristics are included in the model for two reasons. First, some tracts that were not assigned tracts in 1970 are not included in the sample for these estimates. Second, the range of propensity scores populated by both zones and non-zones (the common support region) was smaller when including 1970 characteristics than when conditioning only on characteristics from the 1980's.

Row b of Table 5 has the estimated effects when matching on neighborhood characteristics from the 1970's and 1980's. In California, the results are very similar to those when matching only on characteristics from the 1980's: the effect of enterprise zones on resident employment is not economically significant and is not significantly different from zero. In Florida, the magnitudes of the effects are larger but the pattern across the different outcomes is the same: the effect on raw employment is around -4.0 percentage points while the effect on the neighborhood component of employment is approximately -2.2 percentage points. The estimate of the effect on raw employment is significantly different from zero, but the effect on the neighborhood component of employment is not. For Florida the estimates based on two decades of demographic data are more negative than those based on 1980 data alone, but it is not possible to reject that the estimates are the same. Overall, the estimates are not very sensitive to whether or not I control for longer trends in neighborhood characteristics.

The employment outcomes discussed above were for men and women together. I also study the employment outcomes of men separately. Panel c of Table 5 has the matching estimates of the effect of enterprise zones on the employment rate of men and the neighborhood component of the employment probability of men. These estimates are generated with the same methodology as the pooled results except that the individual sample is restricted to only include men and the matching sample is restricted to those tracts where at least 30 men were included in the employment probability models. The results for these individual-level employment probability models are in

Table 2. The propensity score matching stage results are qualitatively similar to those presented in Table 4 and are not reported.

The estimates of the effect of enterprise zones on men's employment are more negative than the corresponding estimates for the employment of all people in the labor force. For example, the estimate of the effect of California's enterprise program on pooled employment when conditioning on individual characteristics is -0.38 percentage points while the corresponding estimate of the effect of enterprise zones on men's employment is -1.31 percentage points. In Florida, the effect on the raw employment rate is -2.35 percentage points while the effect on the neighborhood component of employment probability is -1.48 percentage points. None of the estimated effects on the neighborhood component of employment probability of male zone residents in the labor force are significantly different from zero.

One potential explanation for why enterprise zones appear to have reduced employment rates is that the programs may have encouraged workers who were out of the labor market to search for work. Suppose that enterprise zones increased the number of jobs available to zone residents and thereby encouraged workers who were out of the labor force to search for jobs. Unless there is an offsetting rise in employment, increasing the number of people searching for jobs would lower the employment measure used above. Therefore, while encouraging workers to enter the labor market may be a benefit to the residents, it can reduce the probability that a resident who is in the labor force is employed.

If it were the case that enterprise zones encouraged people to enter the labor market, one would expect the employment rate of all working age residents to be higher in enterprise zones than comparable areas even though the effect on the employment rate of people in the labor force is ambiguous. To see if this is the case, I estimate the effect of enterprise zones on the proportion of all working age male residents that are employed. I modify the methodology used to estimate the

treatment effects discussed above. The sample restrictions are changed to include men who are out of the labor force and not enrolled in school in the sample; I then repeat the estimation of the employment probability models for the new sample. I focus on only men because it is likely that men aged 18 to 55 who are out of the labor force are either not able to work or have dropped out of the labor market because of inability to find jobs offering more than their reservation wages. I also estimate the propensity score models for the sample of tracts with at least 30 men in the individual-level employment models when non-participants are included. The propensity score stage estimates are very similar to those in Table 4 and are not reported.

The matching estimates of the effect of enterprise zones on the proportion of all working-age men who are employed are in panel d of Table 5. In California, the estimated effect on the raw employment rate of all men ranges is –1.69 percentage points and the estimated effects on the neighborhood component of employment probability is –2.55 percentage points. In Florida, the estimated effects on the employment rate of all men is -3.47 percentage points while the effects on the neighborhood component of employment probability is -1.87 percentage points. These point estimates suggest that enterprise zone programs reduced the probability that a man living in a zone is participating in the labor market and employed. In Florida, the estimated effects of enterprise zones on all men's employment are less negative when conditioning for individual characteristics.

These estimates do not support the view that enterprise zones encouraged workers to enter the labor force. In fact, the estimated effects on the employment of all men, regardless of labor force participation status, are more negative than the estimated effects on the employment of men in the labor force. During the 1980's, many communities experienced rapid growth in the number of men out of the labor from (Altonji and Blank, 1999). While the estimates are not statistically significant, it appears that this phenomenon was more prevalent in enterprise zones than in comparable areas.

The average treatment effect estimates in Table 5 are not without caveats. I estimate the effects from 1987 to 1990 of the enterprise zone programs of two states on zone resident employment. Given the diversity of enterprise zone programs across states, it could be that the same kind of analysis would find different results in other states. It may also be that these same programs would have different effects at another point in the business cycle. The late 1980's were a time of slow growth and the effects may be different during a boom. Because of limited data availability, I study the effects approximately 38 months after implementation. It could be that the long-term effects of enterprise zones are different than the short-term effects I estimate. It is also important to remember that I study only one potential effect of enterprise zones on residents. Enterprise zones may also affect housing markets, as studied by Greenbaum and Engberg (2000), or neighborhood crime, as studied for Business Improvement Districts in Los Angeles by Brooks (2004).

7. Conclusion

This paper provides a comprehensive analysis of the impact of the enterprise zone programs of California and Florida on the employment of zone residents. In order to estimate the effects of enterprise zones on resident employment, I develop and implement a methodology that recognizes the fact that the selection into treatment occurs at the neighborhood-level and that the determination of employment occurs at the individual-level. This methodology is applicable to other topics where the selection into treatment occurs at an aggregated level while the outcome of interest is determined at a less aggregated level. An example would be estimating the effects of an increase in teachers at a school on the test scores of students when there is a selection process that determines which schools receive additional teachers.

The results suggest that enterprise zones of California and Florida had at best no effect on employment and at worst a small negative effect in Florida, but the estimates are not significantly different from zero. Given the substantial hiring tax credits paid by both states in this period, the

negative point estimates are surprising. However, they are in line with prior findings that enterprise zones had little effect on the number of jobs in the zones (e.g. Greenbaum and Engberg, 1999) and findings of small or negative effects on the employment of zone residents (e.g. Papke, 1993 and Greenbaum and Engberg, 2000). It is likely that most of the tax credits paid by the states subsidized hiring that would have taken place regardless of the policies.

Some of the literature that has looked at the effects of enterprise zones on the employment rate of zone residents has found statistically significant negative effects. When looking at the unconditional employment rate of residents, I also find that zone residents were less likely to be employed than residents of observationally similar areas, though in most cases the standard errors are too high to make a definitive conclusion. These results are misleading because they do not control for the characteristics of zone residents. Without exception, once I control for the characteristics of zone residents the estimated effects of enterprise zones on resident employment probability are not significantly different from zero. This is true even though controlling for resident characteristics consistently increases the precision of the estimates.

The enterprise zone programs of California and Florida were carefully targeted and, relative to other enterprise zone programs, provided large incentives for hiring zone residents or people with a history of unemployment. The majority of the zone program expenditures in these states were spent on hiring tax credits. If one were to expect a positive impact of enterprise zones on resident employment, it would be in these two states. I carefully measure zone location, control for the characteristics of people who lived in the zones in 1990, and systematically choose observationally similar non-zone tracts to use as comparison samples. In the end, I find that the enterprise zones of California and Florida had, at best, no measurable impact on the employment of residents after the first three years of the programs. This provides further evidence that, at least at the historical level of

expenditures, enterprise zones are not an effective way of increasing the probability that the residents of distressed communities are employed.

Appendix A: Tract-level Variable Definitions

Not intended for publication

Data source listed in parentheses. The abbreviations used are: 1980 Census of Population and Housing: Summary Tape File 3A (STF80), 1970 Census of Population and Housing: Fourth Count (4C70), and my own tabulations from the Standard Statistical Establishment Lists of 1983-1986 (SSEL).

Variable definitions:

Employment rate: Share of people in the tract-place pair aged 18 to 55 and not enrolled in school who are in the non-military labor force and employed. (STF80 and 4C70).

In remainder place: Place not included in the 1982 Economic Census geography so that the 1982 Economic Census place code equals "9990." (SSEL).

Job growth rate: (place-level employment in 1986 – place-level employment in 1983)/(place-level employment in 1983). (SSEL).

Poverty rate: The fraction of people in a tract-place pair who live in a family with income below 125 percent of the poverty line. (STF80 and 4C70).

Share of adults with more than HS: The fraction of adults in a tract-place pair aged 25 or above with more than a high school degree. (STF80 and 4C70).

Share of workers in manufacturing: The fraction of employed workers in the tract-place pair who worked in a manufacturing industry. (STF80 and 4C70).

Share non-white: 1 minus the share of tract-place pair population that is white. (STF80 and 4C70).

Share single mother households: Share of the tract-place pair households headed by an unmarried female with own children. (STF80 and 4C70).

Unemployment rate: Share of all people in the tract-place pair aged 15 or above who are in the non-military labor force and not employed. (STF80 and 4C70).

Housing vacancy rate: Share of housing units vacant. (STF80 and 4C70).

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Figure 1(a): Distribution of propensity scores in California

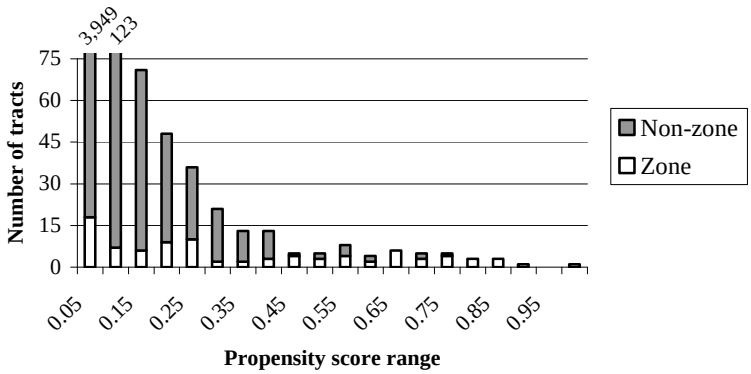
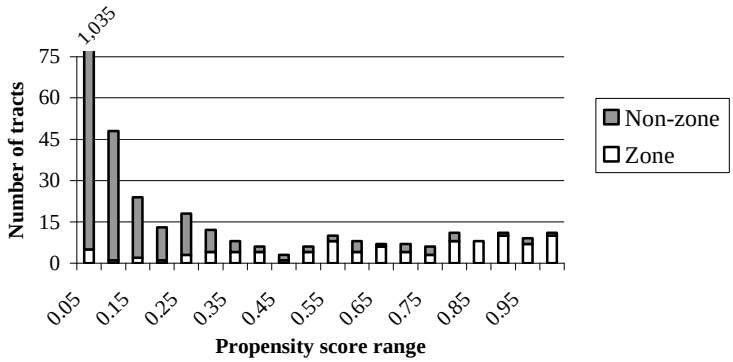


Figure 1(b): Distribution of propensity scores in Florida



Notes: When the number of tracts with propensity scores in the range graphed is greater than 75, the number of tracts is above the bar. The propensity scores graphed are the predicted values from the models in Table 4 for the samples used to estimate those models.

Table 1
Means of individual-level characteristics

	California			Florida		
	In labor force		All	In labor force		All
	Women	Men	Men	Women	Men	Men
Employed	0.936	0.935	0.858	0.942	0.946	0.869
Black	0.070	0.055	0.062	0.147	0.120	0.131
Hispanic	0.218	0.275	0.276	0.130	0.142	0.141
Immigrant	0.010	0.010	0.010	0.009	0.010	0.010
Age	35.916	35.400	35.409	36.122	35.912	36.020
	(9.497)	(9.577)	(9.703)	(9.653)	(9.629)	(9.783)
Age sq./100	13.801	13.449	13.479	13.979	13.824	13.932
	(7.025)	(7.049)	(7.142)	(7.171)	(7.148)	(7.271)
Married	0.559	0.589	0.571	0.565	0.601	0.583
Number of own	0.770	0.801	0.779	0.740	0.709	0.678
children in household	(1.110)	(1.243)	(1.259)	(1.038)	(1.105)	(1.103)
Education level:						
Less than grade 9	0.068	0.104	0.111	0.033	0.047	0.055
Grade 9-12	0.096	0.125	0.137	0.116	0.144	0.155
H.S. Degree	0.237	0.213	0.217	0.325	0.291	0.291
Some college	0.347	0.293	0.286	0.324	0.286	0.279
BA	0.180	0.170	0.161	0.143	0.158	0.151
More than BA	0.072	0.095	0.089	0.058	0.074	0.070
Observations	455,556	551,865	601,427	168,363	174,884	190,321
% of pooled sample	45.2	54.8	48.5	49.1	50.9	46.1

Note: Standard deviations are in parentheses for continuous variables.

Table 2
Employment probability model used to estimate tract fixed effects

	California				Florida			
	Pooled		Men only		Pooled		Men only	
	In the labor force		In L.F.	All	In the labor force		In L.F.	All
	Men	Women		Men	Men	Women		Men
Gender indicator	--	-0.042	--	--	--	-0.051	--	--
		(<.0001)				(<.0001)		
Hispanic	0.005	-0.008	0.006	0.041	-0.003	-0.027	-0.002	0.004
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(0.852)
Black	-0.046	-0.038	-0.044	-0.066	-0.033	-0.026	-0.029	-0.049
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Married	0.038	0.010	0.045	0.093	0.032	0.011	0.039	0.090
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Immigrant	0.002	0.004	0.002	-0.001	-0.006	0.001	-0.005	-0.002
	(0.481)	(0.267)	(0.481)	(0.565)	(0.302)	(0.798)	(0.302)	(0.768)
Age	0.007	0.012	0.011	0.016	0.007	0.012	0.012	0.015
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Age squared *.01	-0.003	-0.005	-0.026	-0.076	-0.003	-0.005	-0.031	-0.106
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Related children in household	0.002	-0.004	0.002	0.004	0.003	-0.004	0.003	0.017
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Education level:								
Less than grade 9	-0.027	-0.063	-0.026	-0.076	-0.030	-0.050	-0.031	-0.106
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Grade 9-12	-0.039	-0.042	-0.038	-0.077	-0.024	-0.038	-0.023	-0.051
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Some college	0.022	0.019	0.024	0.049	0.008	0.011	0.010	0.026
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Associates Degree	0.029	0.030	0.029	0.086	0.019	0.024	0.019	0.054
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
BA	0.035	0.031	0.037	0.083	0.022	0.024	0.022	0.054
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
More than BA	0.040	0.031	0.041	0.092	0.027	0.026	0.027	0.068
	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Individuals	1,007,421		551,865	601,427	343,247		174,884	190,321
Tracts	5,544		5,522	5,538	2,276		2,265	2,272

Notes: Marginal effects are reported. Probability that the underlying coefficients are zero are in the parentheses. The probability that all tract effects are equal is less than 1% for all samples. "In L.F." is an abbreviation for "In the labor force." The "Pooled" models are estimated with men and women in the same model and, in these models, all variables except the tract fixed effects are interacted with gender.

Table 3
Means for tract-level propensity score models

	California				Florida			
	All		Matched		All		Matched	
	EZ	Non-EZ	EZ	Non-EZ	EZ	Non-EZ	EZ	Non-EZ
Characteristics from 1980's								
Log of median household income	0.021	0.674	0.095	0.120	-0.233	0.506	-0.165	-0.064
	(0.317)	(0.389)	(0.307)	(0.339)	(0.332)	(0.337)	(0.337)	(0.319)
Unemployment rate	0.141	0.063	0.127	0.126	0.091	0.049	0.089	0.085
	(0.069)	(0.044)	(0.047)	(0.066)	(0.035)	(0.029)	(0.036)	(0.048)
Poverty rate	0.398	0.138	0.356	0.348	0.455	0.145	0.427	0.399
	(0.140)	(0.101)	(0.117)	(0.146)	(0.143)	(0.102)	(0.150)	(0.142)
Public assistance rate	0.282	0.085	0.234	0.244	0.194	0.050	0.176	0.176
	(0.139)	(0.071)	(0.099)	(0.134)	(0.089)	(0.047)	(0.091)	(0.089)
Share of adults with more than high school	0.173	0.441	0.193	0.220	0.156	0.353	0.166	0.185
	(0.107)	(0.176)	(0.112)	(0.141)	(0.087)	(0.151)	(0.100)	(0.094)
Share non-white	0.641	0.197	0.576	0.549	0.655	0.099	0.610	0.577
	(0.240)	(0.193)	(0.233)	(0.232)	(0.337)	(0.199)	(0.354)	(0.358)
Share single mother households	0.145	0.064	0.120	0.125	0.147	0.054	0.138	0.125
	(0.108)	(0.041)	(0.066)	(0.069)	(0.080)	(0.044)	(0.086)	(0.073)
Housing vacancy rate	0.061	0.050	0.063	0.057	0.098	0.114	0.096	0.096
	(0.038)	(0.056)	(0.036)	(0.066)	(0.058)	(0.107)	(0.055)	(0.071)
Share of units built in prior ten years	0.114	0.271	0.126	0.160	0.147	0.477	0.157	0.164
	(0.147)	(0.259)	(0.155)	(0.210)	(0.116)	(0.277)	(0.124)	(0.119)
Share of workers in manufacturing	0.310	0.200	0.295	0.281	0.133	0.124	0.131	0.142
	(0.136)	(0.101)	(0.131)	(0.133)	(0.079)	(0.061)	(0.074)	(0.066)
Job growth rate	0.145	0.229	0.153	0.184	0.157	0.313	0.184	0.212
	(0.089)	(0.210)	(0.094)	(0.165)	(0.127)	(0.257)	(0.129)	(0.115)
In remainder place	0.231	0.196	0.171	0.177	0.113	0.510	0.196	0.237
	(0.424)	(0.397)	(0.380)	(0.385)	(0.319)	(0.500)	(0.401)	(0.431)
Measures of employment in 1990								
Employment rate	0.842	0.940	0.862	0.858	0.853	0.947	0.856	0.895
	(0.083)	(0.044)	(0.060)	(0.077)	(0.069)	(0.033)	(0.072)	(0.060)
Men's employment rate	0.850	0.940	0.865	0.861	0.863	0.948	0.866	0.905
	(0.078)	(0.047)	(0.065)	(0.088)	(0.076)	(0.039)	(0.070)	(0.072)
Marginal effect from employment probability model	-0.048	-0.006	-0.038	-0.047	-0.041	-0.007	-0.040	-0.022
	(0.052)	(0.031)	(0.039)	(0.051)	(0.043)	(0.025)	(0.045)	(0.036)
Number of tracts	91	4,229	70	62	97	1,164	56	38

Notes : "Marginal effect from the employment probability model" row is the mean of the marginal tract effect evaluated relative to the mean tract effect. See Appendix A for other variable definitions. The "Matched EZ" columns restrict the sample to the zone tracts with a nearest neighbor match when matching on the propensity score estimated with 1980 characteristics. The "Matched Non-EZ" columns restrict the sample to the non-zone tracts that are matched to the zone tracts in the "Matched EZ" columns.

Table 4
Propensity score models

	California		Florida	
	Marginal Effect (hundredths)	Standard Error (hundredths)	Marginal Effect (hundredths)	Standard Error (hundredths)
Log of median household income	-0.014	(0.157)	-0.319	(0.302)
Unemployment rate	-0.361	(0.519)	-0.062	(0.930)
Poverty rate	1.391	(0.746)	1.068	(0.987)
Public assistance rate	1.136	(0.729)	-0.126	(0.545)
Share of adults with more than high sch.	-0.322	(0.257)	-0.490	(0.420)
Share non-white	0.522	(0.238)	0.393	(0.334)
Share single mother households	-0.929	(0.646)	-1.259	(1.177)
Housing vacancy rate	0.211	(0.547)	0.448	(0.571)
Share of units built in prior ten years	-0.007	(0.164)	-0.619	(0.426)
Share of workers in manufacturing	1.269	(0.570)	1.023	(0.882)
Job growth rate	-0.033	(0.188)	-0.774	(0.586)
In remainder place	0.778	(0.568)	-3.690	(1.198)
Observations	4,320		1,261	
Psuedo R-sqaured	0.4714		0.6451	

Notes: The dependent variable is an indicator variable that equals 1 if a tract contains an enterprise zone and 0 otherwise. See Appendix A for definitions of independent variables.

Table 5
Average treatment effect estimates (in percentage points)

Sample and variables included in propensity score model	California			Florida		
	Raw employment	Neighborhood component of employment	Matched zone tracts	Raw employment	Neighborhood component of employment	Matched zone tracts
a. Men and women pooled	-0.601	-0.383	79	-1.521	-1.027	81
	(1.291)	(0.876)		(1.538)	(0.991)	
b. Men and women pooled, conditioning on 1970 and 1980 characteristics	-0.119	-0.268	72	-4.014*	-2.250	64
	(1.255)	(0.851)		(1.870)	(1.155)	
c. Men in the labor force	-1.288	-1.312	69	-2.347	-1.481	62
	(1.083)	(1.027)		(1.855)	(1.194)	
d. All men	-1.693	-2.553	75	-3.470	-1.869	76
	(2.287)	(1.803)		(2.827)	(1.981)	

Notes: Raw employment is the employment rate while Neighborhood component of employment is the marginal tract effect. Except for panel b, the propensity score models for these estimates are in Table 4. The propensity score model in panel b. adds characteristics from the 1970 Census and the sample is restricted to tracts that were defined in the 1970 Census. Boot-strapped standard errors from 300 replications are in parentheses. Results that are significantly different from 0 at the 95% level of confidence are marked with *.

[1] See Busso and Kline (2006) and Krupka and Noonan (2006) for recent analyses of the Federal Empowerment Zone program.
[2] For more detail on the programs, see California Department of Commerce (1987a,b) and Office of the Auditor General (1993).
[3] The vast majority of the credits were claimed by firms hiring zone residents. For example, in 1990 97.7 percent of credits where the class of worker was known were used for zone residents (Office of the Auditor General, 1993).
[4] I consider a tract as designated as part of a zone if the tract contains part of an enterprise zone.
[5] This methodology is related to the regression-adjusted local linear matching estimator introduced in Heckman, Ichimura, Smith and Todd (1998). The key difference is that my method uses less aggregated data to estimate a fixed effect rather than using a residual as the outcome of interest.
[6] Other researchers have applied propensity score matching to enterprise zone evaluation. O'Keefe (2004) uses propensity score matching to find a pool of comparable areas to include in regressions with fixed effects for each matched pair. Bondonio (2000), Bondonio and Engberg (2000), Engberg and Greenbaum (1999), and Greenbaum and Engberg (1998, 2000) use propensity scores to make sample restrictions and as a variable in regressions to control for area characteristics.
[7] To make the estimates from the probit models easy to interpret, I present marginal effects in the tables below. The calculation of the marginal effects is straight-forward for most of the regressors. For dichotomous variables, the marginal effect is the average across observations of the effect of discrete changes. For continuous variables, the marginal effect is the product of the model coefficient and the partial density function evaluated at the sample means. See Greene(1997) for more details on these techniques.
[8] See Black and Smith (2004) for a detailed description of the Epanechnikov kernel estimator.
[9] Following Andrews and Buchinsky (2000), the level of certainty that the bootstrapped standard error is within 10 percent of the standard errors estimated with an infinite number of bootstrap replications is at least 95 percent with 300 repetitions for all matching results presented.
[10] Each probit model with fixed effects for California takes at least 11 hours to estimate. Using the technology available to me (SAS 8.2 running on a Linux workstation with 4 20GHz processors and 16GB of RAM), it would take approximately four months to bootstrap the probit estimation with 300 replications for one sample.
[11] For more information on the geographic units used in the analysis, see the Census Bureau's *Geographic Areas Reference Manual* (U.S. Department of Commerce 1994).
[12] For tracts that are not in a place detailed by the 1982 Economic Census, I use the data for the portion of the county not assigned a place code in the Economic Census.
[13] To reduce measurement error, I drop individuals with imputed data for employment status, enrollment status, race, education, or age from the individual-level regression sample.
[14] See Greene (2002) for recent work on estimation of fixed effects with small samples.
[15] Distance is measured from the population-weighted zone centroid to the population-weighted tract centroid.
[16] The variables I include in the model are similar to those in other papers using propensity scores to control for the enterprise zone selection process, such as Bondonio and Engberg (1999), Greenbaum and Engberg (2000), and O'Keefe (2004). The main exception is that I use city level employment changes rather than local neighborhood changes to avoid measurement error induced by using detailed geographic for establishment data. The variables are also comparable to those in Greenbaum (2004), except I do not include measures of neighboring tracts characteristics.
[17] See Smith and Todd (2005) for an intuitive explanation of why these properties are important.
[18] As detailed in equation 7.